

must remain unexplained; it certainly cannot be found from an inspection of the original manuscripts. But it is proper to remark that there is a considerable discordance between the separate daily results, the seconds of R.A. of the greatest value being 27^s.80, and of the least, 25^s.47, or a range of 2^s.33. The following table exhibits the mean R.A. taken from several catalogues and reduced to 1810, January 1:—

Catalogue.		Mean R.A. 1810, Jan. 1. h m s	Epoch of Catalogue.
Fedorenko's Lalande	..	22 10 29.49	1790
Groombridge (A)	..	26.72	1807
" (D)	..	28.28	1812
Armagh	..	28.93	1840
Oeltzen's Argelander	..	28.98	1842
Radcliffe 1st Cat.	..	28.31	1845
Greenwich 1872	..	28.81	1872

The mean of the R.A. in the two Groombridge Catalogues, A and D, giving a weight to each proportional to the number of observations, is 22° 10' 27".29. The R.A. in the published Groombridge Catalogue is 22^h 10^m 25^s.83.

Kidbrooke, Blackheath,
December 23, 1872.

On an observed Discordance between the Reading for Zenith-point in the Determinations with the Transit-Circle of the Royal Observatory, Cape of Good Hope. By E. J. Stone, M.A. F.R.S., Her Majesty's Astronomer at the Cape.

The Transit-Circle of the Royal Observatory, Cape of Good Hope, was designed by Sir G. B. Airy, K.C.B. Astronomer Royal.

It is similar in construction and optical power to the magnificent instrument of the Royal Observatory, Greenwich. There are only two points of difference. The handles for moving the instrument are removed from all connexion with the reading-circle, to the opposite side of the central cube; and, in the original construction of the instrument, arrangements were made by the Astronomer Royal for the observation of the collimators through the central cube without the necessity of raising the instrument from its Y's.

Sir G. B. Airy has given a full description of the Greenwich instrument, with detailed plans, in the volumes of *Greenwich Observations*, 1852 and 1867. This appears to render any description of the Cape instrument unnecessary. Soon after taking charge of the Observatory, I had observations of α Centauri, β Centauri, and α Eridani, taken by reflexion and directly.

I made it a rule, that with these observations the runs and nadir-point reading with the reflecting eye-piece should be taken by the same observer. A discrepancy of a very sensible amount, between the zenith-points determined from the star-observations and those determined with the reflecting eye-piece, was apparent. In fact, the zenith-point readings, determined with the reflecting eye-piece, were greater by quite one second of arc than the reading determined from the group of stars. The horizontal flexure of the telescope was first redetermined. It differed by nearly $0''.4$ from that in use, which had been determined in 1855; but the effect of this correction would tend to increase the observed discrepancy. The inclination of the wire was carefully examined, but there was no error upon this point. It appeared to me possible that the discordance might be due to imperfect correction for division-errors, connected with some slight deformation of the reading-circle from strains in different positions of the instrument. Some observations of the lengths of arcs, measured under the six principal microscopes and the supplementary microscopes, for the determination of the division-errors, appeared to favour the idea. A complete redetermination of the division-errors to every 5° , and a grouping of residual errors to test this point, was therefore undertaken. The result, however, appeared to prove clearly that the mean reading, with six microscopes, could not be affected from this cause to more than a tenth of a second. The resulting division-errors in the different sets were most accordant; but the observed discrepancy in the zenith-points was but little influenced by their introduction instead of those already in use. The general runs of the old and new determinations were similar, although very sensible differences appeared at certain divisions.

The ground being thus cleared, it appeared probable that this observed discordance must be looked for in an imperfect correction for flexure under the assumed form,—

$$\text{Constant} \times \text{sine (Zenith Distance.)}$$

Some observations of stars by reflexion were then made to the north, for the purpose of a comparison of the results thus obtained with those found from the Southern stars, and from the Nadir observations. The following are the results at present available.

From 143 observations of Southern stars, with mean zenith distance about 25° ,—

$$1. \text{ Nadir Reading—South Horiz. Point} = 90^\circ + 1''.10.$$

From 33 observations of Northern stars, with mean zenith distance about 37° ,—

$$2. \text{ Nadir Reading—North Horiz. Point} = 0''.77 - 90^\circ.$$

The flexure correction employed in the reduction of those observations has been

$$- 0''.26 \sin \text{Z.D. south.}$$

The constant for flexure found in 1871, was $- 0''.617$. If the true correction for flexure is assumed to be—

$$a \sin z + b \cos z + c \sin 3z,$$

we should find from these data,—

$$\text{True Reading} = \text{Reading of Circle} - 0''.376 \sin z + 0''.92 \cos z + 0''.241 \sin 3z.$$

The value of the coefficient of $\cos z$ is so large, that I have endeavoured to control its determination by direct observations. I may mention, that I have carefully tried whether any sensible difference does exist between the nadir-points, determined after moving the instrument in different directions towards the nadir. In the Cape instrument no such discordance appears to exist. There are at the Observatory a theodolite and collimator, with apertures of about an inch and a half. The theodolite telescope does not clear its circle at much greater zenith distances than 150° .

The theodolite and collimator were mounted as opposite telescopes, inclined at angles, 210° Z.D.S. and 30° respectively. The theodolite rested upon a stone slab, across the opening of the transit-circle room. A detached platform was erected for an observer. The collimator was mounted on a wooden frame, resting upon the solid rock; a portion of the observing floor was removed for this purpose. The cross wires of the theodolite were first adjusted to centre wire, free from collimator error, and the horizontal wire of the transit-circle at 30° Z.D.S. The wires of the collimator were then similarly adjusted when the transit-circle was reading 210° . The observer at the theodolite then brought the micrometer wire of this instrument into coincidence with the horizontal wire of the collimator, by vision through the circular apertures of the cube of the transit-circle. When the wires were apparently in coincidence, the object-glass of the Transit-circle was turned upon that of the theodolite, and the instrument clamped, and the readings taken for coincidence of the horizontal wire. The instrument was then turned upon the collimator, and similar readings taken. Finally, as a check, the object-glass was again turned upon the theodolite, which was somewhat exposed, and the readings again taken. If the two sets of readings of the theodolite wire coincidence did not differ much the set was considered satisfactory.

When twenty-one such sets had been made, the theodolite was mounted towards the North, with its telescope at an angle of 150° S. and the collimator was mounted in the pit towards the South, and similar observations were made. Fourteen sets were made in these positions of the instruments.

To avoid the effects of personality as much as possible, the observations at the theodolite and eye-piece of the transit-circle were about equally distributed between me and Mr. G. Maclear. The circle microscopes were read by Mr. Freeman. These observations were continued at favourable opportunities through May 1872.

The results are as follows,—

$$\text{1st Set. Reading for North Collim.} = \begin{cases} \text{That for South Theo.} \\ + 1''.86. \end{cases}$$

$$\text{2nd Set. Reading for South Collim.} = \begin{cases} \text{That for North Theo.} \\ + 2''.15. \end{cases}$$

If we assume, as before, that the flexure is of the form $a \sin z + b \cos z + c \sin 3z$, we should have, since

$$a - c = -0''.617$$

$$\text{True reading} = \text{Reading of circle} - 0''.460 \sin z + 1''.15 \cos z + 0''.157 \sin 3z.$$

The result, therefore, confirms the existence of the large value of b determined from the discordances between the nadir-point readings.

Unfortunately, the only collimating telescopes which I could mount have very small apertures in comparison with that of the transit-circle, and their focal lengths are too small to bear even the power employed. I cannot altogether regard this determination as satisfactory, but I believe that there can be no doubt about the existence of a large term varying as $\cos Z D$. It may be mentioned that the collimator was independently mounted and dismounted eight times during the experiments. I prefer, however, to trust the numerical determination of the constants to the observations of discordance of the zenith-points; but the number of observations of stars to the north is at present scarcely sufficient for an accurate determination of the coefficients, although I believe it to be abundantly sufficient for a very approximate one. It may be remembered that the Astronomer Royal found that the horizontal flexure of the transit-circle at Greenwich changed sign after the piercing of the cube in 1865, and that this change of sign was accompanied by a change of sign in the R—D. correction of the Observatory. I fear that the astronomical flexure of the telescope must be attributed as partly due to changes in the form of the central cube, or parts of attachment of the object-glass and eye-piece cones, and that the crushing and extending forces of these heavy cones are the cause of the existence of the term depending upon $\cos Z D$.

For the reduction of stars very near to the pole, within 5° observed in 1871, I have reduced all the observations with the zenith-points determined from the observations with the reflecting eye-piece, and have then applied to all the results a correction derived in the usual way as a colatitude correction. But, although such a process is satisfactory for stars near the pole, yet if the discordance between the zenith-points is real, and varies as the zenith-distance of the object observed, it is clear that the colatitude deduced, as above mentioned, need not be the true colatitude, and that the stars near the pole and far from the pole cannot be chained together in one series without an allowance for

the existing discordance. I believe this discordance in the Cape instrument must be due to flexure ; but, whatever be its cause, it must be of the utmost importance to keep the discordance well in view, as a check upon assumptions of accuracy which are unobtainable with the instrument, and also in the hope that the cause may be discovered and removed. It is a discordance easily disguised or lost sight of by the use of zenith-point corrections from nadir eye-piece alone.

I cannot, therefore, but regard as most important the clear manner in which my old master, Airy, has insisted upon keeping this point prominently in view, in spite of its having been often regarded, as in one sense it of course is, as an indication of imperfection in the instrument used or the methods adopted. It appears, however, to me that it would be better, in the face of such a discordance, to reduce the observations with nadir-point determinations with the reflecting eye-piece, and to observe stars by reflexion, to detect the law of the discrepancy and the numerical coefficients of its expression. The necessary corrections could then be easily applied to the mean results, and this method would allow of modifications being introduced in the results as further light was thrown upon this subject, or more accurate determinations of the coefficient of an assumed law were obtained.

1872, November 2.

Summary of Sun-spot Observations made at the Kew Observatory during 1872. By Warren De La Rue, Esq., F.R.S.

Months.	Days of Observation.	Numbers given to the New Groups in the Kew Catalogue.		Number of New Groups.	Days without Spots.
January	10	1800	to 1820	21	0
February	14	1821	„ 1843	23	0
March	10	1844	„ 1850	7	0
April	15	1851	„ 1872	22	0
May	18	1873	„ 1891	19	0
June	16	1892	„ 1905	14	0
July	14	1906	„ 1918	13	0
August	14	1919	„ 1927	9	3
September	13	1928	„ 1949	22	0
October	10	1950	„ 1961	12	0
November	13	1962	„ 1979	18	0
December	6	1980	„ 1985	6	0
Total		No. 1800 to No. 1985		186	3

The above, which is a continuation of former tables given in the *Monthly Notices*, has been but partially compiled from the photographs taken with the Kew heliograph.

Since these were discontinued in April last, their place has been supplied for the purpose of group numbering, on Schwabe's method, by rough sketches made from eye-observations with a refracting telescope of 3 inches apertures, using a power of 42.

There is a considerable falling off in the number of days of observation in 1872 as compared with 1871. This is accounted for in some measure by the greater prevalence of bad weather, but principally by the fact that during the chief part of the year, the observer not being exclusively engaged upon Sun-work, was unable to take advantage of temporary breaks in all clouds which offered themselves, the operation of mounting the telescope and making the drawing requiring more time than could always be spared from his other duties.

The system of eye-observation not having been decided upon until after the photographs ceased to be taken regularly, there is no comparative series between the two. There have, however, been several comparisons made at different times which tend to show the general accuracy of the sketches made by Mr. James Foster, one of the junior assistants at the Kew Observatory.

On an Apparatus for connecting the Hour Circle of the Equatorial with the Regulator; and rendering audible the beat thereof. By Wentworth Erck, Esq.

The apparatus I am about to describe has now been in use on my own equatoreal for about a year, during which time it has been productive of so much comfort and convenience, that I have thought others might like to be made acquainted with it.

Its object is twofold,—

First, to keep the hour-circle constantly moving so as to show, at a fixed index, precisely the same time as that shown by the regulator.

Secondly, to render distinctly audible, even to deaf ears, the beats of the regulator.

Both these objects are effected by an electro-magnetic apparatus in which contacts are made by the vibration of the pendulum itself; whereupon the magnet attracts one end of a lever, the other end of which carries a pawl working in a ratched wheel on the axis of the endless screw that drives the hour-circle.

The hour-circle having 720 teeth, and the ratched wheel 120, if the latter receives an impulse every second, the former will revolve once in twenty-four hours; and this is what actually takes place.

The details of the mechanism by which this is effected are as follows: but I must premise that this circle is read in a very unusual way, though I venture to think a very much more convenient way than the usual one.

It is such a height from the floor, that by kneeling you can